December 21, 1949.

Dr. L. Cavalli,
Dept. Genetics,
University of Cambridge,
44 Storey's Way,
Cambridge, England.

Dear Cavalli:

I have just received your strains 123 and 123 Lac-. The latter is almost certainly not my Lac_3^- (of Genetics, 33:617 '48), since your strain ferments glucose, whereas none of the Lac_3^- I have so far isolated in K-12 will do so. Unfortunately, I have not been able to remain in town long enough to do anything with them, but hope to do some testcrosses next month.

Some preliminary experiments with UV on diploids show no striking differences in sensitivity. However, since both the diploids and the haploids are multinucleate, and the latter perhaps more so, this may not be pertinent. At about 10% survival, there is a striking conversion[?— selective killing is not yet excluded] of diploids to haploids. As yet there is little evidence for diploids stabilized by balanced lethals, which may mean that the complete medium actually induces segbegation, rather than merely permitting the growth of auxotrophic segregants. However some multiple dominant combinations may be occurring, which are rare on a recombination basis and these may be actually diploid. By diploid and haploid above I meant only the v aadapposed to + orv- colony type on EMB medium, and this may need further consideration.

I have a letter from Lominski requesting coli strains, or rather permission for same. Please feel free to send out these strains generally to anyone who wants to work with them, although I would like to learn who is interested. However, for the strains carrying Het , I would prefer a direct request. This might well be to mutual advantage, as we hope to be developing improved strains with the passage of time.

The factor 100x for Hfr/Nfr accords with my own "intuitive" conclusions. I hope that you will be able to obtain derived Hfr strains from the S^r x Az^r with which you can further test the oppositional character of Hfr. As far as salts go, Davis and I have found dome discrepancies in frequency of "plate recombination", which he believes now to be due to differences in minimal medium composition, which he has been evolving for some time. However, in an experiment I did, no effect of Mg or citratebcould be discernedm although I am still convinced that Mg is important.

On the reverse side is the formula for Davis' current minimal medium.

This formula is quite good, giving dense growth without aeration (owing to high bugger capacity). However, recombinants coming up on this medium often tend to be mucoid, for reasons I do not well understand. Conceivably, this might have something to do with lysogenesis.

Per liter:

K_2HPO_4	7
KH_2PO_4	2
Na_3citrate.$5H_2O$	.5
$MgSO_4.7H_2O$	.1
$(NH_4)_2\ SO_4$	1

Autoclave separately

Agar as required
Glucose 1 (sic).

The diploids are cytologically distinguishable from haploids, but so far uninterpretably.

Sincerely,

J. Lederberg